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Dear Professor Buchner:

Professor E. A. Steinhaus of the University of California has given me your current address, and suggested that it might be worthwhile for me to write to you.

I should say first that I have been specializing in the genetics of microorganisms, especially the bacteria. Lately, my wife and I have been particularly interested in the genetic aspects of the endosymbiosis of bacteriophages with bacteria (lysogenic bacteria). From this entry, it has become obvious that the genetical significance of "hereditary" symbioses has been grossly overlooked. I need simply refer to the "kappa" system in Paramecium and the "genoids" of Drosophila, in addition to lysogenicity, as instances where a familiarity with the biology of endosymbioses may shed a great deal of light on remarkable genetic problems.

About a year ago, the journal "Physiological Reviews" solicited a review on the genetics of microorganisms. I was not enthusiastic about another summary of the same material (e.g. in Heredity, Sept. '48; Ann. Rev. Microbiology, 1949) but I thought that a discussion of "cellular genetics", i.e., a reexamination of the cell theory in the light of findings in microbial genetics, would be useful. It soon became apparent that classical nuclear behavior (which has been the bulk of my work with Escherichia coli) adds no critical novelty to such a discussion; it would have to revolve around the numerous cases of inheritance via autonomous, extranuclear factors. From this, it is easy to see the transition to endosymbioses.

This letter is prompted by the information (supplied by Dr. Fraenkel of the University of Illinois) that you are preparing a new book on endosymbiosis. I have depended so heavily on your 1930 "Tier u. Pflanze in Symbiose" that I could scarcely ignore this news, even in anticipation. If you plan to include a discussion of the bearing of endosymbiosis on genetics in your new book, my review will clearly be superfluous, and I will be pleased to abandon it. The manuscript is presently in a very rough form, so there will be little effort lost. If this subject is not to be a significant theme of your book, there may still be some point to my own interpretations. I must draw so heavily on illustrations from fields with which I am not directly familiar that the review will be much less useful if it does not take advantage of your own summary. The article has been promised for submission by July 1952, but this can be postponed if it appears to be advantageous. May I then ask the following few questions:

1) do you plan to comment on genetic work on "cytoplasmic inheritance" in the light of endosymbiosis? 2) If not, can you tell me the publication plans to help me decide whether to postpone my own review article? 3) Is there any possibility of a discussion of your book prior to its publication, or of seeing an advance or proof copy in time to help my own schedule? If you found this feasible, for example, could I use a chapter outline to frame a few questions on the scope of your discussion where it might overlap mine, or to ask for a few more recent references. The general questions in which I would be most interested are: 1) determination of phenotypic characters by symbionts (e.g. in Pseudococcus brevipes - Carter's work) and 2) effects of apo-symbiosis (i,e, disinfection), and especially of substitution of microsymbionts from other species (the only reference I can find is Fraenkel's brief paper on Stegobium and Lasioderma). By way of one specific question, has there been any work on "Syncyanom" since Pascher's very stimulating 1929 paper?

I realize the imposition involved in these questions, and will be most gratified at your consideration of them. I am looking forward to your book with great anticipation. Biology may congratulate itself on the continuation